Dilatation of the Iris

In addition to the method of observation mentioned by Mr. Ackroyd in his photometric proposal (Nature, vol. xxi. p. 627) I may mention that the variations of the diameter of the pupil are very beautifully observed by a pair of punctures in a screen over the eye. In fact long ago I used this as a means of observing the absolute diameter of the pupil, subject to a small unexamined constant error.

By pricking a row of holes in a card at distances of ·06, ·07, ·08 . . . . ·25 inch, and placing this close over the eye, the diameter is observed by sliding the card until two of the holes are found at such a distance that their edges appear to touch. The opening of the other eye, or the slightest disturbance of light, produces an apparent alteration in the sizes of the disks of light, so that their edges recede or overlap; and a fresh pair of holes may be found showing the altered diameter of the pupil.

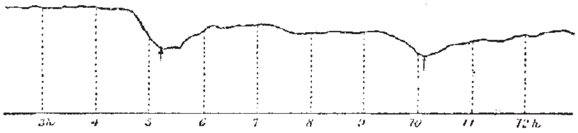
Thus (1) the extreme diameters of the pupil were found to be ·07 (? ·06) and ·25 inch; (2) the diameter is rapidly variable at will, *without any alteration of stimulus*, even as much as from ·13 to ·19 inch; (3) the sympathetic motion takes ·4 or ·5 second to be produced when the other eye is exposed to light. Many similar interesting questions may be examined by this simple instrument; for which purpose I inclose a sample card for editorial trial.

W. M. F. P.

Comparative Curves in Terrestrial Magnetism

Monsieur,—En séquence à la intéressante communication de Rev. S. J. Perry de l'Observatoire de Stonyhurst, je vous prie d'additioner les suivants renseignements sur la même perturbation magnétique de 17 mars passé, d'après la courbe du déclinographe de l'Observatoire du Infant D. Luiz, à Lisbonne.

La différence entre le maximum et le premier minimum à 5h. 49,5m. G.M.T. est de 13′,9 (un tiers du mouvement à Stonyhurst), et entre le même maximum et le 2e minimum à 10h. 45m. G.M.T. est un peu plus grande 16′,2, le contraire qu'on voit à Vienne et Stonyhurst.



Declinographe 17 Mars, 1880 (Lisbonne M.T.).

Il est digne aussi de remarque que le temps du 1er minimum ne s'accorde avec le temps à Stonyhurst et Vienne, pendant que le temps du second est de parfait accord.

La longitude de cet Observatoire est + 36m. 35s. G. Je vous envoie la copie de la courbe.

Agréez, Monsieur, l'assurance de ma haute considération.

Lisbonne, 21 juin 1880 J. Capello

Effects of Lightning on Trees

Your note in Nature, vol. xxii. p. 204, on the recent thunderstorm at Geneva induces me to send you a note on a tree struck by lightning in Stoneleigh Park during a severe storm on last Thursday week (June 24). The tree was a fine oak about forty feet high, and the lightning seemed to have struck not among the smaller branches at the top, but about two-thirds of the way up the main trunk, just where several of the larger branches came off from the stem. From this point to the ground the bark had been rent off along a strip about three inches wide, and through the whole length the wood beneath the bark had been gauged out as if by a carpenter's tool, the groove made being about an inch wide and deep. The curious fact of the tree being struck apparently among the branches at once suggested to me that the electricity must have travelled, without visible effect, through the upper branches, and only produced disruption of the wood when the current was strengthened by the combination of a great number of separate streams. I had forgotten that this was Prof. Colladon's theory of electric discharge, but am glad to be able to give it the support of this observation.

Rugby, July 3 L. Cumming

Iron and Hydrogen

In the description given a few weeks back of the experiments of Prof. Hughes, the fact was demonstrated that iron wire in contact with dilute acids becomes brittle, and at the same time takes up hydrogen.

There are one or two points of great interest that many, perhaps, besides myself, would like to know more about.

Thus, at the same time the iron becomes brittle, does it also become harder?

This leads one to speculate on the facts illustrated in the hardening and tempering processes of steel.

We know that such liquids as water, weak acid, oil, &c., which are used as baths in which the heated metal is quenched, are all decomposable by iron and other metals at a high temperature, the result being the liberation of hydrogen, &c. Now is it not probable that this liberation of hydrogen is really the essential element in the physical change produced in the hardened steel?—that is to say, that the steel absorbs, or perhaps becomes alloyed with the nascent hydrogen in contact with its surface, thus rendering it intensely hard?

Prof. Hughes has pointed out that a red heat entirely dissipates the hydrogen from the iron wire, which returns to its normal state.

This perhaps will explain the process of tempering by supposing that a certain proportion of the (hardness-rendering) hydrogen is driven off according to the temperature reached, as shown in the well-known shades of colour seen on the surface—that is to say, the hardness is proportionate to the contained hydrogen, such as that many other metals become very hard or soft by being alloyed as zinc and copper in brass, tin and copper in bronze, &c.

This is supported by the fact that one of the most successful processes of hardening depends on the use of a quenching-bath of dilute sulphuric acid. This would be explained by the greater ease with which acidulated water is decomposed by iron, and therefore a larger bulk of the nascent hydrogen liberated on its surface could be absorbed by the metal.

The carbon in steel probably only plays the part of a go-between in rendering the absorption of hydrogen more facile. There is a fact that also supports this, namely, if unhardened steel is dissolved in HCl the carbon is left in the form of graphite scales, whereas after hardening, if treated with the acid in the same manner, the residue is found to consist of a liquid hydrocarbon, thus showing the presence of hydrogen in the metal.

These points I should like to have been able to confirm or refute for myself; but not having the required time or apparatus, I leave it with the hope that some one possessing those advantages will settle these questions.

H. J. Johnston-Lavis

Naples

"Coronella lævis"

In Nature, vol. xxii. p. 156, the presentation is announced of two specimens of *Coronella lævis* (British) to the Zoological Society. I have known so many persons doubt the existence of the *Coronella* in the New Forest that I should feel greatly obliged to any of your correspondents who would give me some information as to its history, whether it has been introduced, or is really indigenous.

H. King

[Mr. Sclater tells us that he has no doubt that the smooth snake is indigenous to the British Islands, although it was overlooked for many years. The first living example received by the Zoological Society was in August, 1862, presented by Mr. Fenton, having been obtained in the neighbourhood of Sandhurst. Since then nearly twenty specimens have been received, chiefly from the New Forest and neighbourhood of Bournemouth. See Mr. Cooke's excellent little volume, "Our Reptiles" (London, 1865), for a full account of this species of snake.—Ed.]

Recall of Appearance of Books, &c.

I have only to-day been able to read the back numbers of Nature for the past two or three months, and hence have only now seen Mr. Ernst's letter in your issue of April 29 last.

His power of recalling the appearance of books I know is possessed by others. I have a very large and still increasing library, but there is hardly a volume, or indeed a tract, the appearance and condition of which does not at once present itself to my mind if occasion to use it should arise. Further, being engaged in the compilation of a work some years since, wherein many references to other books were necessary, I used,

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when away from home (as was frequently the case) to write and indicate not simply in what part of the library the book would be found, but in what portion of the volume, and almost always whether on the left or right hand page, any given passage required would be found.

Of late years I have found it desirable to rebind my tracts in something of a uniform manner. Their external individuality is thus destroyed, but the aspect of their title-pages and the location of particular passages of the contents remain as fresh as ever. CORNELIUS WALFORD

London, June 24

Stags' Horns

WITH further reference to the above question I have pleasure in inclosing a letter received to-day from the head keeper at Bradgate Park, near Leicester, where both red and fallow deer are kept.

I may add that I saw at the end of July last, near the head of Loch Eribol, in Sunderland, a quantity of stags' horns in a gipsy encampment, which I supposed had been collected for sale by that curious fraternity. HERBERT ELLIS

62, New Walk, Leicester

"*To* HERBERT ELLIS, ESQ.

"*Bradgate Park, 22nd June*, 1880

"DEAR SIR,—In answer to yours of the 19th inst. respecting what becomes of the stags' horns after being shed, I beg to say they are regularly collected and sold. But there is not the slightest doubt of their eating each other's horns. I have myself seen several cases where both brow antlers and the top points have been gnawed off. I have also seen Scotch heads that have been quite spoiled by the tines having been gnawed, which must have been done after the horn had become hard, and whilst the animal was living. I am, sir, yours respectfully,

"C. OVERTON"

Cup and Ring Stones

MANY of the markings mentioned by Mr. Middleton are hollows made by rain, or rather deepened by rain-water holding many low organisms in hollows, on the upper surfaces of exposed grit stones; overflow from these accounts for the groove or spout noticed at the margin of some of them. They are to be seen on the stones erected near Boroughbridge, and speak to the length of time these stones must have been raised into their present position. W. S.

June 21

Diatoms in the London Clay

To enable me to determine the exact extent of the diatomaceous band in the London clay, I am anxious to obtain information of any wells in progress, or in contemplation, anywhere in the London Basins, *west* and *north* of London. With the help of some of your readers I have no doubt that I shall shortly be able to show that the one referred to is co-extensive with the London clay. The details I wish for are:—

1. Locality of well.
2. If begun, the depth attained.

I shall also be glad to hear of any railway cuttings now being made in the same area. W. H. SHRUBSOLE

WATER SUPPLY

AMONG the improvements in sanitary matters that this generation has witnessed not one ranks higher than the settled and still growing conviction of the importance of a pure water supply, and nowhere are the various aspects of the question more keenly debated and considered than in the Metropolis at the present time.

At a discussion at a recent meeting of the Chemical Society there seems to have been some doubt thrown on the conclusions arrived at by chemists in determining the wholesomeness of a water by no less an authority than Prof. Huxley, and it may be well to inquire how far his allegations are borne out by facts.

In the earlier days of the history of chemistry, as was to be expected, the processes adopted in the analysis of water were crude in the extreme, and the quaint ideas promulgated in the treatises then published are not a little amusing. Gradually, however, and especially during the last few years, the methods of analysis have improved, and although, judging by the wide diversities of opinion that exist as to what may or may not be pronounced a water sufficiently pure for drinking purposes, the subject cannot yet be said to have arrived at a stage completely satisfactory; still, so far as the purely chemical evidence is concerned, it would seem to be able to furnish results which are sufficiently exact for all practical purposes. The operations involved are among the simplest and easiest the chemist has to perform, and consequently it is not the data furnished by analysis that are called in question, but the conclusions drawn from them.

Persons interested in sanitary questions, but who have no special knowledge of the difficulties that beset the forming a correct judgment as to the wholesomeness of water, are apt to express themselves as scandalised, and it must be confessed with some show of reason, that it should be possible there should be so little agreement amongst those who are looked up to as authorities on such matters.

This disagreement, however, is more or less inevitable in the present state of our knowledge, and is largely due to the intricacy of some of the problems involved in the question, which is by no means a simple chemical one.

The debatable ground is the nature and estimation of organic matter and the amount of significance that should be attached to the presence of oxidised nitrogen compounds.

Organic matter may be of animal or vegetable origin, the former being dangerous and the latter much less so, if indeed it be not altogether innocuous. To distinguish between the two kinds is therefore all important; but unfortunately it is impossible directly to do this, as both animals and vegetables yield albuminoïd matters, which are, chemically speaking, practically identical in composition.

Of the various processes for the estimation of organic matter there are three that are in general use. One, the oldest, known as the permanganate process, finds its advocate in the present day in Dr. Tidy, and consists in measuring the organic matter by the quantity of oxygen required to oxidise it. Another, originated by Prof. Wanklyn, and which he calls the albuminoid-ammonia process, consists in decomposing the organic matter by an alkaline solution of potassium permanganate, and taking the resulting ammonia as the measure of the organic matter. The third process, the one employed in the laboratory of the Rivers Pollution Commissioners and advocated by Dr. Frankland, its originator, estimates the organic carbon and nitrogen separately.

A good deal may be said in favour of all these processes, as affording a rough estimation of the quantity of organic matter, but none of them can be relied upon as giving any indication of its nature, *i.e.*, as to whether it is dangerous or not; and yet it is the almost invariable custom to judge of a water by the quantity of organic matter it contains, no matter what its origin, and a variation of two or three times a given amount is held to make the difference between a good and bad water.

It was to this point that Prof. Huxley especially addressed himself in his remarks already referred to. He gave it as his opinion, speaking as a biologist, "that a water may be as pure as can be as regards chemical analysis, and yet, as regards the human body, be as deadly as prussic acid, and on the other hand may be chemically gross and yet do no harm to any one." "I am aware," said he, "that chemists may consider this as a terrible conclusion, but it is true, and if the public are

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